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TITLE

The long-term health effects of attending a selective school: a natural experiment

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BACKGROUND

Education is associated with physical and mental health and life expectancy in countries around the world ~~Education is strongly associated with health~~, and the role of education in improving health is a focus of diverse research and global policy[1–6]. However, there is vigorous debate about the causal effects of educational reforms. Because children from socio-economically advantaged families are more likely to attend better-~~-~~resourced schools, be educated longer, and gain more ~~and better~~ qualifications, the association between education and health may simply reflect ~~students’~~ background advantages rather than effects of educational policy itself[4, 7–9].

Studies seeking to identify the true causal effects of education on health have focused on evaluating increases in length of schooling. They take advantage of policy decisions that create “natural experiments”, or environments that mimic the conditions of a randomised trial[10]. One such natural experiment is a nationwide increase in the age at which a child may legally leave school, which allows the comparison of the health of the cohorts just before and just after the policy was implemented. Analyses of these natural experiments across multiple countries have shown that length of compulsory education causes small but significant reductions in mortality, smoking, and obesity[11–14].

The long-term effects of other educational reforms have received less attention. Here we consider the policy of stratifying students based on their academic ability, referred to as selective schooling. Historically most developed nations stratified students by ability either into separate schools or classes, but since the 1960s many countries moved to less ~~selection~~ stratification [15, 16]. Recently, however, the policy has come back into debate, and across OECD nations there have been reforms both towards a more stratified system and away from it[17]. In the United Kingdom, the educational system moved from selective to comprehensive more than forty years ago[18, 19], but the present UK government has pledged a return to a stratified system in an effort to reduce the achievement gap between children of different socioeconomic circumstances[20].

Previous ~~analysis~~ analyses of selective schooling’s effects have focused on its impact on educational trajectories and long-term economic outcomes[21–24]. In contrast, selective schooling’s effects on health have been less well explored. The mechanisms by which selective schooling may ~~improve~~ change health are proposed to be both direct, via ~~increased~~ health literacy, as well as indirect via ~~increases~~ changes in prosperity, status, and control[9, 25, 26]. However, while ~~S~~studies have demonstrated an association between selective schooling and health, ~~but~~ the results have been shown to be subject to confounding. Recent work has attempted to model the association more robustly by controlling for a wide~~-~~ variety of covariates and matching exposed and unexposed individuals[27–30]. However, in addition to the potential of residual confounding, these studies evaluated only self-reported health measures derived from participant survey. The results are hampered both by surveys only through mid-life, and by high attrition over time leaving a non-representative population.

Here we take advantage of a natural experiment in school assignment that occurred in Britain in the 1960s, ~~and~~ as well as the data sharing policy of the National Health Service of Scotland to evaluate the long-term causal effects of selective schooling on health. Our study population is 8,087 people who were assigned to selective or non-selective secondary school based on their performance on an exam taken at age 11. They are part of the Aberdeen Children of the 1950s, a long-running birth cohort which holds extensive data on participants from across their lives[31, 32]. As this population was one of the last to be assigned to school via exam, and as they are now over 60, they provide an excellent opportunity to evaluate long-term health effects. We show that a regression discontinuity design which alleviates the bias due to residual confounding is appropriate for estimating the effect on health of attending a selective secondary school. We consider a wider range of outcomes than previous studies, including self-reported physical health and mental health, diagnoses of 30 chronic conditions from hospital records, and premature death. We consider health at two periods: when participants were in middle age and early old age.

METHODS

Study Design

This study will use a regression discontinuity design to investigate the effect of selective secondary schooling on health[33, 34]. In such a design, allocation to ~~the~~ an exposure depends on the value of a continuous variable (the forcing variable). In this study, assignment to selective schooling ~~depends~~ depended on an exam score at age 11.

The design uses the fact that if there were no treatment effect, there would be a smooth relationship between the forcing variable and the outcome. In this study, if selective secondary schooling has no effect on health, the distribution of health outcomes will be smooth across scores. However, if there is a discontinuity in the outcomes at the forcing variable cut-point (i.e. if health outcomes change suddenly in people with scores close to the cut-point), the effect can be attributed to selective schooling.

Data Sources

This is an observational study using secondary analysis of data from the Aberdeen Children of the 1950s birth cohort study linked to routine national health care data. There will be no interaction with study participants and no new data will be gathered. The data requested from the ACONF study and the NHS are described in detail below and listed in Appendix A.

Approvals

The study protocol has been publicly registered[35]. This protocol has been approved by the Aberdeen Children of the 1950s study steering committee (ACONF046), the NHS Scotland Public Benefit and Privacy Panel for Health and Social Care Research (1718-0208), and the Administrative Data Research Network research approvals panel (PROJ-143).

Data Access

We have access to the exposure data, including participants' test scores, secondary schools, demographic and socioeconomic data. We have used these data to determine if a natural experiment appropriate for analysis using a regression discontinuity design exists (presented in the Pilot Data section below).

We do not have access to the outcome data. These data have been linked to the exposure data by the Electronic Data Research and Innovation Service of NHS Scotland[36] and are currently held in the National Services Scotland National Safe Haven[37]. Upon in-principle acceptance of this study, we will publicly register the approved registered report at Open Science Foundation, and then access the outcome data.

Exposure

The exposure is attendance at a selective secondary school.

In the UK at this time, there were two types of state-run secondary schools: junior secondaries (called secondary modern schools in England) designed to prepare students for training and work, and senior secondaries (grammar schools in England) designed to prepare students for further education at university and professional careers[38].

Students were allocated to secondary school based on their performance on a series of tests taken in the final year of primary school, referred to as the 11+ exam[19]. For ACONF participants, those with 11+ exam scores below 540 were assigned to junior secondary schools; those with scores at or above 560 were assigned to senior secondaries; and those in between were considered individually[39].

The ACONF study holds secondary school attended for most participants, and it holds the five components of the 11+ score for all participants (described in detail below).

Outcomes

We will measure six outcomes:

1) overall health score (self-reported in 2001)
2) mental health score (self-reported in 2001)
3) presence of limitation due to health problems (self-reported in 2001)
4) number of chronic diseases (from hospital records 1997-2001)
5) number of chronic diseases (from hospital records 2011-2015)
6) risk of death by age 60 (from national death records)

The first three outcomes are held by the ACONF study and were collected by postal survey of the cohort in 2001 when they were ~~in their~~ between 46 and 51 years old[32].

The two chronic disease burden outcomes will be derived from hospital and mental health admissions records (NHS Scotland Information Service Division datasets SMR01 and SMR04[40]). Diagnoses of any of the chronic conditions validated by Tonelli[41] will be counted. Two 5-year windows will be analysed, corresponding to the participants' mid-life (46-51 years old) and early old age (60-65 years old).

Date of death is held by the ACONF study and is continually updated from the death certificates at National Records of Scotland[32].

Study Hypotheses

H0) Selective secondary school attendance is not associated with self-reported overall health in mid-life

H1) Selective secondary school attendance is not associated with self-reported mental health in mid-life

H2) Selective secondary school attendance is not associated with limiting health problems in mid-life

H3) Selective secondary school attendance is not associated with chronic disease diagnoses in mid-life

H4) Selective secondary school attendance is not associated with chronic disease diagnoses in later life

H5) Selective secondary school attendance is not associated with risk of death by age 60

ANALYSIS PLAN

Variable Processing

All of the variables we will receive are from the ACONF study or from NHS hospital admissions records. They are shown in Appendix A.

Schooling

Primary school attended is defined as the school attended in 1962 (variable RS028). Secondary school attended is available from the postal survey in 2001 (variable Q1094), with the oldest students in the cohort also having it recorded from their education records in 1964 (variable RS095). Analysis shows that there are no discrepancies between sources, so both will be used. Selective schools are defined as the three state-run schools which required a high 11+ exam score to attend and the three fee-paying schools which also required a high admissions test score to attend. The 11+ exam score is the sum of variables RS053-RS057.

Self-rated Health

Overall health score is from a self-rated four-point scale (variable Q1001), and limitations of daily activities due to health from a self-rated presence/absence (variable Q1002). Mental health is from a self-rated four question survey, each with four-point scale. The four responses (variables Q1016-Q1019) will be summed to one value corresponding to mental health.

Chronic Disease Diagnoses

Processing of hospital and mental health admissions records will be done by the Information Services Division (ISD) of NHS Scotland, as data protection laws require. ISD will link ACONF study participants via their Community Health Index number to the national hospital and mental health admissions datasets SMR01 and SMR04. Each admission record includes diagnoses coded according to the International Statistical Classification of Diseases (ICD)[42]. We have supplied ISD with Tonelli's set of chronic diseases and their validated ICD diagnosis codes[41] (Appendix A). For each individual, ISD will return a binary variable for each disease indicating presence/absence of any of the ICD codes in that disease's set within the given five-year window (1997-2001 or 2011-2015). We will sum the number of chronic diseases diagnosed to one variable. ISD will also provide a variable indicating if the participant had no Community Health Index number and therefore could not be linked to medical records.

Death

Age of death will be derived from the date of death (variables D_DMON and D_DYEAR) and the age in months in December 1962 (variable RS002).

Covariates/Controls

The following will be used as covariate/control variables: sex (RS001), IQ at 9 (RS180), father's occupation (R040), and completion of a higher degree (Q1098b). Father's occupation has previously been classified using The Registrar General's Classification of Occupations 1950[39]. Father will be considered a manual labourer if his occupation is coded III (skilled manual), IV (semi-skilled manual) or V (unskilled manual).

Study Population Exclusions for Exposure

The 11+ exam and assignment procedure was changed the year the youngest students in the ACONF cohort took it, so we will exclude these students (Table 1). We will also exclude students who were already enrolled in primary schools that were selective, religious, for special needs, or for children in care, as these students were highly likely to stay in the same type of secondary school regardless of their 11+ scores (Table 1). Finally, we will exclude students who did not take all five parts of the 11+ exam (Table 1).

The population that remains is designated the **Base Sample** (n = 8,087), made of all ACONF cohort members assigned to secondary school based on the same 11+ test score criteria.

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Table 1. Exclusions from the total ACONF population for this study

population	excluded	reason
12148	2513	in youngest school year (different exam)
9635	216	in private primary
9419	531	in selective state-run primary
8888	260	in Roman Catholic primary
8628	183	in special needs primary
8445	76	in special care outside Aberdeen
8369	282	lack 11+ scores
8087		**Base Sample**

Secondary school attendance is known for 5112 (63%) of those in the Base Sample. This population is designated the **Study Sample**. There is no discontinuity in this attrition across the 11+ exam score cut-off, indicating it will not have an effect on the analysis (Figure 4 in Pilot Data). We exclude from the main body of the analysis the remaining 2,975 people for whom secondary school is not known, but we will impute school attendance for this sub-sample based on their 11+ exam scores and use this as a sensitivity analysis (described below).

Study Population Exclusions for Outcome

Lack of survey response and NHS dataset limitations means each outcome will not be available for all participants. We will address this limitation by considering multiple outcomes. 7,144 of the total ACONF population (65%) replied to the survey which is the source of the self-reported health outcomes[31]. Hospital admissions are available for those registered with NHS Scotland, previously shown to be 11,106[43]. Death records include participants in the UK and those who were reported from abroad for all years since study inception.

Probability of responding to the 2001 survey is smooth across the 11+ exam score cut-point (Figure 4 in Pilot Data), and we will do equivalent tests for probability of linkage to NHS hospital admissions records.

Regression Discontinuity Design

The study is based on the assumption that in the absence of an effect of selective schooling, there would be a smooth relationship between the 11+ exam score and health. In contrast, the presence of an effect of selective schooling would be seen as a discontinuity at the score cut-point – a change in the mean level of health between people who scored just to either side of the cut-point.

We designed the analysis method based on recommendations in Cattaneo, Idrobo and Titiunik[44].

Functional form

We will model the schooling-health effect using a non-parametric, linear approximation local to the score cut-point. This method considers a bandwidth of scores around the cut-point and fits ~~two~~ a regression function~~s, one for the control population and one for the treatment~~for each population on either side of the cut-point, giving more weight to observations closer to the cut-point. The difference in outcome due to selective schooling is then the difference of the intercepts at the cut-point. It is preferred to the global, parametric approach which is vulnerable to overfitting and inappropriate influence from points distant to the cut-point.

Bandwidths and Weights

This method requires rigorous selection of the bandwidth, which determines the data to which the functions are fit. Bandwidth will be chosen automatically using the mean square error optimal method[45]. We will use the triangular kernel function to assign weights to the observations within the bandwidth.

Two-stage Least Squares Estimation

For this population, the probability of attending selective secondary school ~~does not increase to 1 immediately at the 11+ exam cut-point~~does not map perfectly to exam score (Figure 1 in Pilot Data), meaning some students who had scores over ~~540~~ 560 did not go to a selective school and some with scores under 540 did, with the result considered a “fuzzy” discontinuity. This is equivalent to non-compliance in a randomized controlled trial[34].

To account for the fuzziness of assignment, we will estimate the effect of selective schooling as the ratio of the effect given the probability of attendance (equivalent to local average treatment effect estimation in randomized controlled trials)[34, 46]. We will use the two-stage least squares method, where the first stage will be a model of the probability of attending a selective school contingent on exam score, the cut-point and their interaction. This probability of treatment will then be used instead of a binary treatment variable in the second stage.

Inference

We will use the robust bias-correction inference method[47].

Outputs

For each of the six health outcomes the following results will be presented:

1) Graph of the regressions of the outcome across 11+ exam scores, with scores given as the average within a quantile bin
2) A table showing: the bandwidth sizes used; the population size in the bandwidths; the effect estimate; the p-value; the confidence intervals

Quality Control Tests and Sensitivity Analysis

To determine if the study as designed represents a valid natural experiment that could be analysed for a regression discontinuity, we have conducted a series of quality control tests using pilot data (described in detail in Figures 1 – ~~5~~ 4 in Pilot Data below).

These tests confirmed:

Figure 1) a large discontinuity in selective secondary school attendance at the 11+ score cut-point

Figure 2) smooth 11+ score density across the cut-point

Figure 3) no difference in pre-treatment covariates between populations on either side of the cut-off

Figure 4) no difference in likelihood to reply to the postal survey between populations on either side of the cut-off ~~treatment effect on a negative control outcome (postal survey reply)~~

Once we have access to the outcome data and have generated the effect estimate for each of the six outcomes, we will test the robustness of the estimate by examining its sensitivity to variations in the analysis. For each outcome we will report additional effect estimates for: 1) variation in bandwidth selection, 2) higher-order polynomial form, 3) inclusion of covariates (sex, IQ score at age 9, father's occupation), ~~5~~4) exclusion of the population immediately around the cut-off, ~~6~~5) inclusion of those for whom secondary school attended is not known (imputed by inverse probability weighting using the 11+ score).

Power

An analysis of statistical power for regression discontinuity designs suggests that to detect a small difference (0.2 of a standard deviation) between two means at the 2.5% level (one-tailed) with 95% power would require a sample size of 1753, with 80% power for a sample size of 1078. For a large effect size (0.8 of a standard deviation) the sample sizes were 109 and 68 respectively. These results suggest that regression discontinuity designs' sample size is around 2.7 times higher than the corresponding RCT[48].

Average self-rated health in the ACONF study has been reported as 3.01 with a standard deviation of 0.78[49]. For a small effect (0.2 of a standard deviation) at the 5% level with 80% power and taking account of the relative size of the exposure groups, the regression discontinuity sample size applying the correction is 2762, for a large effect (0.8 of a standard deviation) it is 201.

This suggests that our sample is adequately powered to detect relatively small effects. In our analysis we will assess the results in totality rather than focussing on the achievement of statistical significance at a given level on any one test. We will assess whether effect sizes are consistent across outcomes and robust across sensitivity analyses, whether the magnitude of the effect sizes is plausible, and whether it is in line with other studies.

Software & Data

All analysis for this study will be done in R. All code and packages used for analysis and generation of figures and tables will be published. Data on individuals in the study cannot be made publicly available due to data protection laws, but all data will be archived in the National Safe Haven for 7 years and will be available to analyse there.

TIMELINE FOR COMPLETION

Upon in-principle acceptance, the outcome data for this study will be accessed, and we estimate 6 months from release to finish all analyses and resubmit the manuscript.

PILOT DATA

To determine if the study as designed represents a natural experiment that could be analysed for a regression discontinuity, we conducted a series of tests of appropriateness using pilot data on socioeconomic status and schooling.

The most important tests for assessing the viability of regression discontinuity design for a natural experiment are around the integrity of the forcing variable, in this case the 11+ exam scores. 11+ scores are a strong predictor of selective secondary school attendance in the Study Sample (Figure 1), and there is a steep discontinuity at the expected point.

Figure 1. 11+ exam score and the probability of attending selective secondary school in the Base Sample (n = 8087). Scores are in bins of 5 points. Scores below 350 or above 650 not shown due to small numbers. Score cut-offs of 540 and 560 are shown in red.

The density of the test score should be smooth across the cut-point, indicating there was no manipulation of the scores to influence attendance. Distributions of the 11+ assignment scores are smooth across the range determining secondary school assignment, and a McCrary density test did not detect a discontinuity (p = 0.21).

Figure 2. Distribution of 11+ exam scores for Base Sample (n = 8087). Scores are in bins of 5 points. Scores below 350 or above 650 not shown due to small numbers. Score cut-offs of 540 and 560 are shown in red.

Two further tests of the comparability of the populations on either side of the score cut-off were done. For a valid design, covariates determined before the 11+ exam (sex, IQ at age 9, father's occupation birth) should be smooth across the assignment cut-point, as discontinuities in these predetermined variables would call into question the validity the forcing variable[44]. Here ~~Likewise, there should be no systematic differences in key covariates for students just below and just above the cut-off.~~ IQ at age 9 is smooth across the range of 11+ exam scores (Figure 3) as is the probability of father working as a manual labourer and probability of being female.

Figure 3. 11+ exam scores and mean IQ in the Base Sample (n = 8087). Scores are in bins of 5 points. Scores below 350 or above 650 not shown due to small numbers. Score cut-offs of 540 and 560 are shown in red.

~~To test a placebo outcome where we expect to see no effect, we~~We also evaluated if there was a discontinuity in the likelihood to reply to the 2001 postal survey, which is the source of secondary school attendance as well as three of the health outcomes. The overall trend was an increase in likelihood to reply as 11+ scores increased, but the relationship was smooth across the 540–560 range (Figure 4).

Figure 4. 11+ exam scores and the probability of replying to the 2001 postal survey in the Base Sample (n = 8087). Scores are in bins of 5 points. Scores below 350 or above 650 not shown due to small numbers. Score cut-offs of 540 and 560 are shown in red.

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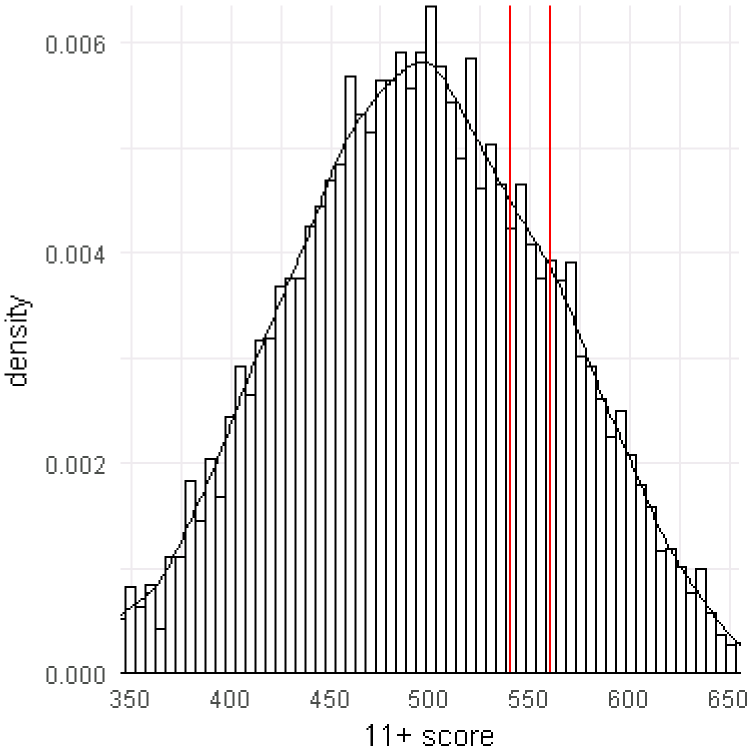

Figure 3

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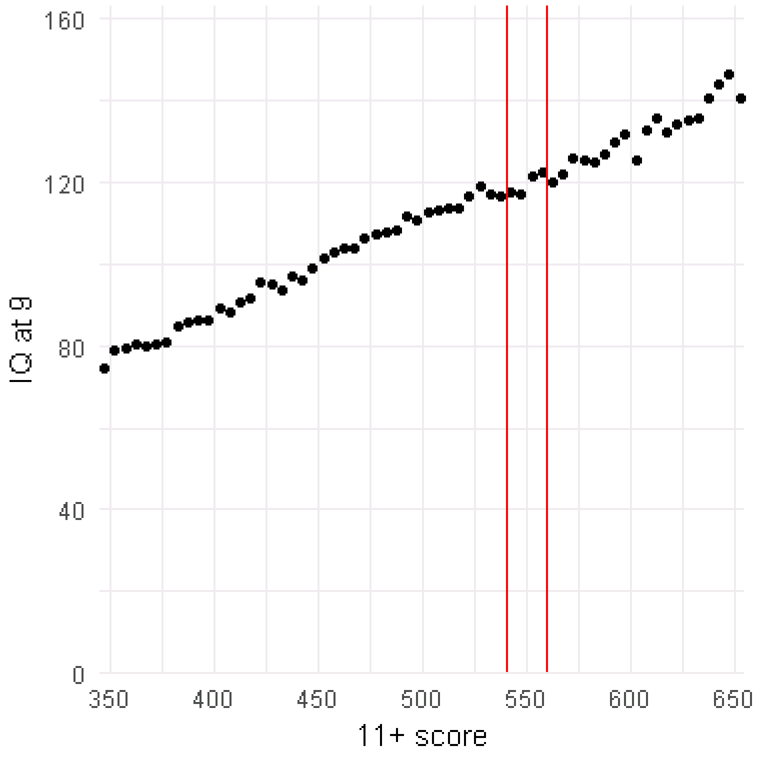

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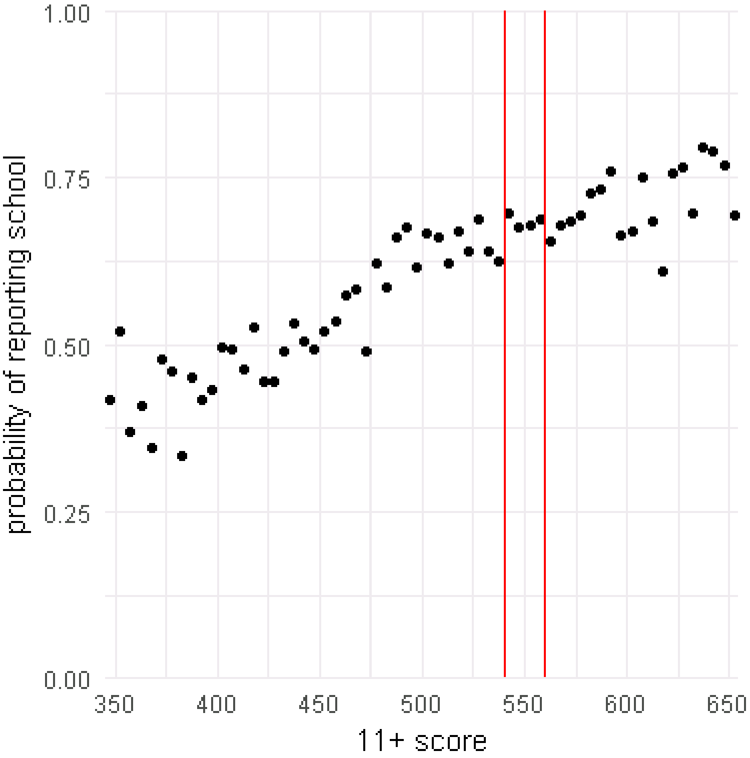

Figure 1

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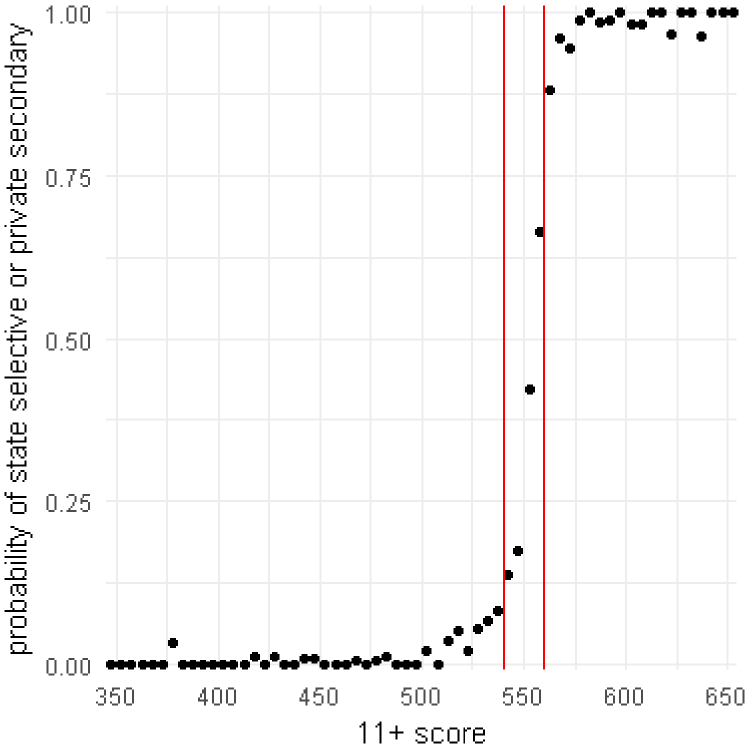

Author's Response to Reviewers' Comments
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Response to reviewers of "The long-term health effects of attending a selective school: a natural experiment", BMED-D-18-01470

We would like to thank the reviewers for their thoughtful and helpful comments. Below, reviewer reports are given in bold with our response and the changes in the manuscript immediately beneath. We have also made minor corrections to typographical errors. All changes are tracked in the attached manuscript, except the addition of three references (#4-6) because this caused change tracking throughout the manuscript, making reviewing difficult.

Reviewer #1: This is a clear description of the research design and analysis with attention to all details that are important to describe for pre-specification of the study. It is also a notably innovative study in terms of the sound identification strategy, and the proposed inference from the data is appropriate. The pilot data is well used to support the validity of the proposed analysis. I have no changes to suggest.

Reviewer #2: In general, I think this is an elegant study design, which takes advantage of the excellent opportunities for data linkage in Scotland and allows long-term follow up of study participants with good retention rates.

Specific comments:
1. The authors provide a good overview of the existing literature relating to individual-level associations between education and health, and to the impact of increases in compulsory schooling on health outcomes. It might be useful to include some literature on country-level effects of education on health.
Viner RM, Hargreaves DS, Ward J, Bonell C, Mokdad AH, Patton G. The health benefits of secondary education in adolescents and young adults: An international analysis in 186 low-, middle- and high-income countries from 1990 to 2013. SSM Popul Health. 2017 Dec;3:162-171. doi: 10.1016/j.ssmph.2016.12.004.

We agree and have rephrased the opening paragraph and added three additional references (#4-6) (page 1 from line 25).

2. I think the manuscript would benefit from reviewing the assumptions and language around how selective schooling might have an impact on health (eg page 2, line 25). I couldn't see any mention of potential negative effects of selective schooling - either on those who passed or didn't pass the 11 plus exam. Possible adverse effects might result from being separated from primary school friends (physically and/or socially) and the potential impact of failing an exam on self-esteem, confidence and mental health. Similarly, the language used in page 10, line 6 about a control and treatment group doesn't reflect that fact that both groups have received an intervention which may have an impact on their short and long-term health (ie being categorised as successful or unsuccessful academically).

The proposed analysis does not presuppose a direction of effect for selective schooling, and we agree that these phrasings are inaccurate. We have made the requested changes to the manuscript (now page 3, line 1 and page 10, line 11) as well as to three other points that have similar language (page 2, lines 3-4; page 3, line 2).

3. Please could you clarify the apparent discrepancy between a cut-off of 540 (as described on page 10, line 19) and two separate cut-offs of 540 and 560 on page 5, line 14.

The goal of the text on page 5 is to describe the school assignment procedure. The goal of the text on page 10 is to show that compliance with assignment was imperfect, however the first number on line 25 is incorrect – it should be 560 not 540 – which has muddled the meaning.

This has been corrected and the text rephrased to: "the probability of attending selective secondary school does not map perfectly to exam score (Figure 1 in Pilot Data), meaning some students who had scores over 560 did not go to a selective school and some with scores under 540 did, with the result considered a "fuzzy" discontinuity.".

4. Page 7, line 2. Can children who changed schools during the secondary school period be identified? If so, how are these individuals dealt with?

For the most part, no. The source of secondary school attended is self-report in 2001 for 4835 (95%) of the Study Sample. School attended is also known from education records collected in 1964 for a minority, so for 325 people in the Study Sample we have school attended from two sources. There are no discrepancies between the sources (page 7).

5. I found the findings of figure 3 and figure 4 were a little difficult to follow - partly due to separate descriptions on pages 11 and 13/14. Clarifying the wording and/or restructuring might make the descriptions easier to follow for the reader.

Both figures are used to show the comparability of the populations on either side of the score cut-off. The format of the registered report requires an awkward separation of the description of data and analytic strategy from the pilot data and figures. These will be combined in the manuscript for publication as: Validation of the Regression Discontinuity Design.

On page 11 we have added a pointer to the Pilot Data section (lines 16-17) and rephrased point 4 (line 4). On page 14 (from line 13) we have added further description of the motivation for figures 3 and 4 and a reference (#44), as follows:

For Figure 3: "Two further tests of the comparability of the populations on either side of the score cut-off were done. For a valid design, covariates determined before the 11+ exam (sex, IQ at age 9, father's occupation birth) should be smooth across the assignment cut-point, as discontinuities in these predetermined variables would call into question the validity the forcing variable (44)."

For Figure 4: "We also evaluated if there was a discontinuity in the likelihood to reply to the 2001 postal survey, which is the source of secondary school attendance as well as three of the health outcomes. The overall trend was an increase in likelihood to reply as 11+ scores increased, but the relationship was smooth across the 540–560 range (Figure 4)."